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ADDRESS

DELIVERED AT

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THE ANNIVERSARY MEETING

OF THE

GEOLOGICAL SOCIETY OF LONDON,

On the 20th of FEBRUARY, 1874;

PREFACED BY

THE ANNOUNCEMENT OF THE AWARD

OF

THE WOLLASTON MEDAL,

THE PROCEEDS OF THE DONATION-FUND,

AND THE MURCHISON MEDAL

AND GEOLOGICAL FUND

FOR THE SAME YEAR.

BY THE DUKE OF ARGYLL, K.T., D.C.L., F.R.S.,
PRESIDENT OF THE SOCIETY.

LONDON:

PRINTED BY TAYLOR AND FRANCIS,

RED LION COURT, FLEET STREET.

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PROCEEDINGS

AT THE

ANNUAL GENERAL MEETING,

20TH FEBRUARY, 1874.

AWARD OF THE WOLLASTON MEDAL.

In handing the Wollaston Gold Medal to the Foreign Secretary, W. W. SMYTH, Esq., for transmission to Prof. HEER, of Zurich, the President referred to the fact that last year the Council had awarded the Balance of the proceeds of the Murchison Geological Fund to Prof. Heer, and remarked that it gave him much pleasure that the Wollaston Medal, the highest honour which the Society had in its power to confer, should be so worthily bestowed. He alluded briefly to the labours of Prof. Heer in the difficult departments of Fossil Botany and Entomology, and to the admirable works in which he had given to the world the results of his indefatigable researches.

Mr. W. W. SMYTH, in reply, said :—

MY LORD PRESIDENT,

It is with great pleasure that I undertake the transmission to Prof. Heer of this new testimony of the importance attached by this Society to his long-continued labours.

I have received from our valued Foreign Member a letter stating that my announcement of the award had found him extended on the bed of sickness, and begging me to assure the Society that, but for this misfortune, nothing would have given him greater pleasure than to have been present at this Meeting, and to have thanked the Society personally for the high honour which has now been awarded to him.

AWARD OF THE WOLLASTON DONATION-FUND.

The President then presented the Balance of the proceeds of the Wollaston Donation-fund to the Foreign Secretary for transmission to Dr. H. NYST, of Brussels, remarking that this distinction had been

well earned by Dr. Nyst by his admirable researches upon the Molluscan and other fossil remains of his native country.

Mr. W. W. SMYTH briefly thanked the President on behalf of Dr. NYST.

AWARD OF THE MURCHISON MEDAL AND GEOLOGICAL FUND.

The President next presented the Murchison Medal to Dr. J. J. BIGSBY, F.R.S., F.G.S., and remarked, in so doing, that there was a peculiar fitness in this award, which would have met the approval of the distinguished geologist in accordance with whose last wishes this Medal was given. It was awarded to Dr. Bigsby in recognition of his long and valuable labours in that department of geology and palæontology with which the name of Murchison is more particularly connected.

Dr. BIGSBY replied, thanking the Society for the honour conferred upon him, and the President for the terms in which he had spoken of his labours.

The President then handed half the Balance of the proceeds of the Murchison Geological Fund to R. ETHERIDGE, Esq., F.R.S., F.G.S., for transmission to RALPH TATE, Esq., F.G.S., expressing a hope that it would be regarded by him as a testimony of the value set by the Society upon his palæontological researches, especially on the Fauna of the Lias, and that it would enable him to enlarge the sphere of his investigations.

Mr. ETHERIDGE, in reply, read the following letter of acknowledgment from Mr. TATE:—

“MY LORD PRESIDENT AND GENTLEMEN,

“To say that I am unworthy of the honour that you have awarded me by the bestowal of the ‘Balance of the Proceeds of the Murchison Fund,’ would be to call into question your judgment, and would render nugatory its value to me.

“The encouragement that such an award conveys is ample recompense for labour bestowed in palæontological research, and is a real incentive to more diligent work. It is in this spirit that I accept the award, and tender my warmest thanks to you for the distinction it confers.

“It is now twelve years since I was led to select for special study

the geological history of the Lias, which appeared to me not to have received that attention at home that it had upon the Continent, and which it claimed by offering the earliest phase of Mesozoic life, and presenting a number of physical problems that seemed, upon the threshold of the inquiry, to reward even the casual observer with a rich harvest.

“I have published from time to time fragments relating to the stratigraphy and palæontology of this period; but I hope soon, in conjunction with my friend Mr. J. F. Blake, F.G.S., to submit, in a work entitled ‘The Yorkshire Lias,’ a comprehensive review of the chief characteristics of the period, embracing the remarkable variation of mineral conditions, and the particular distribution of organic life, as indicative of peculiarities of depth of ocean, the direction and proximity of land, &c.

“Despite all these efforts, the ambition to acquire the position of an expositor of the life of this interesting group of strata urges me to the completion of a *Prodromus* or *Thesaurus Liassicus*, the materials for which have been accumulated during several years; but, from the great labour demanded to bring into harmony the nomenclature of the fossils, without which the compilation can have no real value, some time must elapse before the results can be submitted to you.

“Faithfully yours,

“RALPH TATE.”

“20 High Street, Redcar,
February 19, 1874.”

The President then presented to Mr. ALFRED BELL the other half of the Balance of the proceeds of the Murchison Geological Fund, and stated that this was awarded to him in recognition of his valuable researches upon the fossils of the newer Tertiary beds of this country, and to assist him in the completion of his work upon the Crag deposits of the eastern counties.

Mr. BELL, in reply, said that he was most grateful for this token of the Society's appreciation of the value of his labours, and stated that up to the present time he had been enabled to distinguish about 3000 fossil species from the newer Tertiaries of Britain, and that he hoped yet to add very largely to their number.

THE ANNIVERSARY ADDRESS OF THE PRESIDENT,
HIS GRACE THE DUKE OF ARGYLL, K.T., F.R.S.

GENTLEMEN,—It has always been the practice of those who have occupied the Presidential Chair of this Society to preface their remarks either upon the progress of geological science during the year which has just elapsed or on some more special subject of geological interest, by brief biographical sketches of some of those who during the preceding twelve months have been removed by death from the sphere of scientific activity*.

JOHN WICKHAM FLOWER, elected a Fellow of this Society in 1863, was born in London on the 11th of August, 1807. From an early period he manifested a strong taste for the study of geology and archaeology; and he spared neither trouble nor expense in adding to his collections, which were always at the disposal of his fellow students. He collected fossils largely from the Tertiary cliffs of Hampshire and the Brick-earth at Gray's, in Essex; and for many years he worked hard at a problem which he set himself to solve on his first going to reside at Croydon, about twenty-five years ago—namely, to ascertain whether the great pebble-beds of Addington, belonging to the Lower Tertiary series, were not composed of flints derived from beds of chalk higher than any which now remain in the vicinity of London. His researches seemed to favour this view; but, unfortunately, their results have never been published.

When the questions relating to the antiquity of man began to attract attention, Mr. Flower, who was doubly interested in them both as a geologist and an archaeologist, entered with much zeal into their investigation; and the later years of his life were chiefly devoted to the study of the prehistoric remains of man. He collected stone implements energetically, and speedily amassed a very fine series of those objects. His views with regard to the theoretical conclusions to be derived from the mode of occurrence of the old flint implements of the Drift differed somewhat from those entertained by other geologists; they are to be found in his last published paper, "On some recent Discoveries of Flint Implements of the Drift in Norfolk and Suffolk, with observations on the theories accounting for their distribution," which appeared in the twenty-fifth volume of our 'Quarterly Journal.' Mr. Flower contributed several other papers on

* The following notices have been prepared by the Assistant Secretary.

stone implements to our Journal and to the 'Journal of the Anthropological Society.' His other writings consist of a lecture "On the Pleasure and Advantages to be derived from the Study of Natural History," published in 1857, of one or two papers on Archæological subjects, and several works of a theological nature.

Mr. Flower was attacked by his last illness while staying at Rome in the spring of last year; he hastened home, and died almost immediately after his arrival, on the 11th April, 1873.

Sir HENRY HOLLAND, Bart., M.D., D.C.L., F.R.S., who was elected a Fellow of this Society as early as 1809, was born on October 27, 1788, at Knutsford, in Cheshire. He studied medicine in the University of Edinburgh, where he took his degree in 1811. In 1815 he was elected a Fellow of the Royal Society, and in 1828 a Fellow of the Royal College of Physicians of London. Having been appointed Physician in Ordinary to the Prince Consort in 1840 and to the Queen in 1852, Dr. Holland was created a Baronet on the 18th April 1853. He received the Honorary degree of D.C.L. from the University of Oxford in 1856, and was also an LL.D. of the University of Cambridge, Massachusetts. For several years he was President of the Royal Institution of Great Britain. Sir Henry Holland's published writings are not numerous, the pressure of his professional duties probably preventing him, especially during the later part of his life, which terminated on the 27th of October last, from devoting much time to science. In early days he contributed to our 'Transactions' "A Sketch of the Natural History of the Cheshire Rock-salt District," which appeared in the first volume (1811); in 1816 he published, in the 'Philosophical Transactions,' a paper "On the Manufacture of Sulphate of Magnesia at Monte della Guardia, near Genoa." His 'Travels in Albania, Thessaly, &c.,' also contain some natural-history information. His other writings chiefly relate to medical and physiological subjects.

Dr. GEORGE ORMEROD, F.R.S., F.S.A., one of our earliest Fellows (having been elected in 1823), died on the 9th October last. He was the posthumous and only child of Mr. George Ormerod, of Bury, in Lancashire, the representative of a family which had resided at Ormerod in the same county since the time of Edward III. Dr. Ormerod was born in Manchester on the 20th October, 1785. He was educated at Chester, and at the age of 18 entered Brasenose College, Oxford, where he took his degree of Master of Arts in

1807. He early showed a very strong taste and great aptitude for antiquarian research; and the work by which he is best known, his 'History of Cheshire,' was prepared by him during the ten years after his quitting the University. It was published in three folio volumes in 1819. Dr. Ormerod does not appear to have written any thing on geological subjects.

HENRY CURWEN SALMON, a mining engineer of some note, was elected a Fellow of this Society in 1859. He was born in Dublin on the 21st June, 1827. As his father was largely interested in the mines of Wicklow and Clare, Mr. Salmon from an early age enjoyed unusual advantages for the study of metalliferous deposits and the mode of working them. He afterwards removed to Cornwall, where he remained for several years, taking up his residence in various parts of the country as they suited his researches. The earlier results of his investigations appeared in the 'Mining Journal;' in 1865 he commenced the publication of the 'Mining and Smelting Magazine,' which contained many original and translated papers of great scientific and practical value; but as it failed to meet with adequate support, it was soon discontinued. During his residence in Cornwall Mr. Salmon communicated to this Society his observations "On the Occurrence of Large Granite-boulders at a great depth in West Rosewarne Mine, Gwinear, Cornwall," which appeared in vol. xvii. of our 'Quarterly Journal.' He also took an active part in the proceedings of various provincial Societies. In 1863 Mr. Salmon paid a professional visit to Nova Scotia, and in 1869 to California. On his return from the latter expedition his health failed; and after a long and painful illness he died at Southport on the 28th April, 1873.

JAMES GARTH MARSHALL, elected a Fellow of this Society in 1833, inherited from his father a love of science, especially of geology, for the study of which his residence among the mountains of the north of England afforded him many facilities. These he turned to good account during the many years in which he resided in his paternal house at Hallsteads on Ulleswater, where he made and recorded a valuable series of meteorological observations, including the rainfall on Swarthfell, and afterwards at Coniston. He traced the Bala or Coniston limestone across the country near his home, and paid close attention to the metamorphic and granitic rocks of the district, his observations leading him to an opinion

which has been adopted by many geologists, namely that the so-called granitic nuclei are often only the completely changed portions of the neighbouring strata, there being often between the wholly reconstituted granite and the original stratified mass some incompletely altered rocks of the kind usually termed metamorphic. He brought forward some experimental results in confirmation of his opinion, which he also supported by a considerable experience in the volcanic regions of Europe. Mr. Marshall's papers on these subjects will be found in the 'Reports of the British Association' for the years 1839, 1858, and 1861. During the latter portion of his life Mr. Marshall resided in the neighbourhood of Leeds, where he occupied a prominent position, and took much interest in the prosperity of the local scientific institutions. He died in October 1873.

SIR WILLIAM TITE, C.B., F.R.S., F.S.A., the only son of Mr. Arthur Tite, a merchant of London, was born on the 7th February 1801. On leaving school he was articled to Mr. Laing, the architect to the Custom House, and was afterwards in extensive practice as an architect. Many of the earlier railway-stations were designed by him; and in 1840 he came prominently into public notice as the architect of the new Royal Exchange. He was elected a Fellow of the Royal Society and of this Society in 1835, and a Fellow of the Society of Antiquaries in 1839. For some years he was President of the Architectural Society and of the Royal Institute of British Architects. He sat in Parliament for the city of Bath since June 1855; he was knighted in July 1861, and made a Companion of the Bath in 1870. Sir William Tite was the author of several papers published in the 'Archæologia,' and of a descriptive Catalogue of Roman Antiquities found in the site of the Royal Exchange, the latter privately printed. He does not appear to have written any thing upon geological subjects.

By the death of LOUIS JEAN RODOLPHE AGASSIZ science has been deprived of one who probably occupied a greater place in the eye of the world than any naturalist since the days of Cuvier and Alexander von Humboldt; and if we consider the work that he effected, and especially the influence which he exerted by his residence in the United States upon the scientific development of that country, it is hard to say that the estimation in which he has been held was at all exaggerated.

Born on the 28th May, 1807, in the Parish of Mottier, between the Lakes of Neuchatel and Morat, of which his father was the

Pastor, Agassiz was sent, at the age of 11, to the Gymnasium at Bienne, and at 15 to the College of Lausanne, where he remained for two years. On leaving Lausanne he made choice of medicine as his profession; and having studied for two years in the Medical School of Zurich, he proceeded in 1826 to the University of Heidelberg, where he studied anatomy and physiology under Tiedemann, zoology under Leuckart, and botany under Bischoff. In the autumn of 1827 Agassiz removed to the University of Munich. Here he formed the acquaintance of Oken, Martius, Schelling, and Döllinger, and became the leading spirit in a small society of young men who met for the discussion of scientific subjects. The debates of this society, which was known in Munich as the "little Academy," were of such interest that the Professors of the University did not disdain to assist at them.

Agassiz took his degree of Doctor of Philosophy at Erlangen in 1829, and in the same year that of Doctor of Medicine at Munich. On the latter occasion he presented as his thesis a Latin dissertation bearing the title *Femina humana superior mari*.

During all this period Agassiz was actively engaged in zoological investigations. His earliest paper is the description of a new species of *Cynocephalus*, published in Oken's 'Isis' for 1828; but his chief attention was directed to ichthyology, and especially to the fishes of the European fresh waters; and the same volume of the 'Isis' contains his first ichthyological paper, a description of a new species of the genus *Cyprinus*. His qualifications as an ichthyologist were so highly esteemed at this time, that on the death of Spix he was requested by Professor Martius to undertake the description of the more remarkable species of fish collected by Spix and Martius during their travels in Brazil. These descriptions were published as part of their great work in the year 1829.

Agassiz then commenced the preparation of a 'Natural History of the Freshwater Fishes of Europe,' the researches connected with which occupied him more or less for many years. Questions arising in connexion with his study of existing fishes led him to the examination of the fossil forms which occur at Eningen and Glarus, in Switzerland, and at Solenhofen, in Bavaria; and his attention having been thus turned to fossil fishes, he entered on their study with great ardour, and speedily became the first authority on fossil ichthyology. In collecting materials for the great work on this subject which he afterwards published, Agassiz visited the principal museums of Europe, and from time to time gave some of the results of

his investigations in the form of memoirs communicated to various journals. He was enabled to visit Paris by the liberality of a friend of his father's, a clergyman named Christinat, who, having come into the possession of a small sum of money, voluntarily offered it to Agassiz to aid him in his scientific pursuits. In Paris he made the acquaintance of Cuvier and Alexander von Humboldt. The former was so delighted with the zeal of Agassiz and with some drawings of fish which he had brought with him, that he offered to hand over to the young Swiss naturalist all the materials which he had accumulated towards his great work on Fishes.

In 1832 Agassiz returned to Switzerland and was appointed Professor of Natural History in the newly reorganized University of Neuchatel. In 1833, by the liberality of Humboldt, he was enabled to commence the publication of his greatest palæontological work, the '*Recherches sur les Poissons fossiles*,' to which he had devoted the labour of seven years. In this magnificent book, which consists of five volumes of text and about 400 folio plates, beautifully executed by Mr. Dinkel*, Agassiz described and figured about 1000 species of fossil fish, and gave short indications of about 700 more. The '*Recherches sur les Poissons fossiles*' appeared at Neuchatel between the years 1833 and 1844. During the investigations made for its production, Agassiz found the existing ichthyological system deficient in many respects, and especially inconvenient for palæontological purposes. He accordingly proposed a new classification of fishes, founded on the nature of the scales and other dermal appendages in those animals; but this system has not stood the test of later researches, although one of the orders proposed by Agassiz, that of the Ganoids, has been adopted, with some modifications, in modern classifications.

In 1839 and 1840 Agassiz at last brought out the work on the Natural History of the European Freshwater Fishes, the preparation of which had led him in the first instance to the study of fossil ichthyology. In this beautiful book the species are admirably described and figured, and a great amount of information upon the anatomy and development of some of them is given. In the latter department of his work Agassiz received important assistance from another distinguished Swiss naturalist, Carl Vogt. The expense of publishing this work was so great that it caused Agassiz considerable pecuniary embarrassment for several years.

* The original drawings for these plates are in the possession of the Society, having been presented by the late Earl of Ellesmere.

The splendour of his researches upon fossil fishes and of the magnificent work in which he gave their final results to the world has thrown into the shade the other palæontological labours of Agassiz at this period, although these were of themselves sufficient to establish for him a very high scientific reputation. His miscellaneous palæontological writings consist of important memoirs upon various groups of echinoderms and fossil shells, some of them prepared in conjunction with his friend M. T. Desor. He also published, under the title of '*Nomenclator Zoologicus*,' a classified list of all the names employed in zoology, up to the date of its publication, for genera and groups of higher systematic value, with references to the authors who first proposed them and the works in which they were established, and indications of the derivation of the names. The publication of this work (which must have cost him enormous labour) was commenced in 1842 and completed in 1848. The '*Bibliographia Zoologiæ et Geologiæ*,' an alphabetical catalogue of works published on those sciences, was originally prepared by Agassiz for reference in his own work; it was afterwards enlarged and published in England by the Ray Society between 1848 and 1854.

As if all these labours, together with the duties of his Professorship, did not furnish him with sufficient occupation, Agassiz during this period also devoted much attention to the phenomena of glaciers and of glacial action, especially the nature of the movement of glaciers, and the traces of the former existence of such ice-streams in places where nothing of the kind is now to be seen. The distribution of boulders over the plain of Switzerland had been ascribed by De Saussure to the agency of water. The notion that glacier-ice could have any thing to do with the transportation of boulders seems to have originated with the Chamois-hunters, who noticed that even large masses of rock were annually moved by glaciers. This idea was adopted and extended by Venetz and Charpentier, who attributed the transport of boulders across the valley to the slopes of the Jura to the former extension of glaciers far beyond their present limits. Upon this point Agassiz appears to have been at first rather sceptical; but Charpentier convinced him of the reality of ice-action, both in the transport of boulders and in rounding and polishing rock-surfaces, by taking him up to the glacier of the Diablerets and there demonstrating to him the effects produced by the ice. Agassiz was then led to estimate the quantity of ice necessary to fill up the great valley between the Alps and the Jura to the depth required for the transportation of boulders from the former

to the height at which they are found upon the latter; and he inferred from the results of his investigation that the production of such a vast mass of glacier-ice necessitated a period of cold which must have covered the whole north of Europe with ice. His glacial hypothesis was first announced to the Helvetic Society in 1837; but he continued his investigations for years, and in search of traces of ice-action visited not only the countries in the vicinity of Switzerland, but also England, Scotland, and Ireland. From 1836 to 1845 Agassiz spent his summer vacations among the high Alps, chiefly on the glacier of the Aar, engaged in the investigation of the movements of glaciers. The results of his researches were published in his '*Etudes sur les Glaciers*' (1840) and his '*Système Glaciaire*' (1847).

In the year 1846 Agassiz went to America, and landed at Boston in October of that year. His account of the origin of his expedition to America, given in a letter to Professor Silliman dated 1st February, 1846, is as follows:—"Knowing the great desire I had to visit your country," he says, "and the impossibility of doing it at my own expense, his Excellency, the Baron von Humboldt, who has always treated me as a friend, and whose good counsels have been to me like those of a father, proposed to the King of Prussia to give me the necessary funds for the journey, which His Majesty granted to me in the most generous manner, furnishing me with a sum sufficient for a journey of two years, travelling alone." His object in visiting North America was to study the geology and natural history of that continent; and at the same time he accepted an invitation from Mr. J. A. Lowell to lecture at Boston. He first delivered a series of lectures at the Lowell Institute on the Animal Kingdom, and immediately afterwards a second series on Glaciers and their former extension. Having completed this engagement, he visited New York, Philadelphia, and Charleston for the purpose of investigating the littoral faunas of the north and south of the United States. Early in the summer of 1847 Prof. A. D. Bache, then Superintendent of the U.S. Coast Survey, offered Agassiz the privilege, of which he afterwards availed himself to good purpose, of making free use of the vessels employed by the Survey. Agassiz was so delighted with the facilities for investigation thus presented to him that he determined at once to take up his abode permanently in the United States. He made many dredging-expeditions in the surveying vessels, the last being a great exploration on board the '*Hassler*,' in which he worked along both shores of the continent of

South America and the Pacific shores of Central America and the United States. The results of this voyage have not yet been published.

At the end of 1847 the scientific school at Cambridge, Massachusetts, was established by Mr. Abbot Lawrence, and Agassiz was appointed Professor of Zoology and Geology there. He commenced his duties in the spring of 1848. At the close of the session he made an excursion with twelve of his pupils to make a scientific exploration of the shores of Lake Superior, the results of which appeared in a large 8vo volume. This book, which was the joint production of the party, contains the chief results of the observations made by Agassiz on the distribution of boulders and on glaciated surfaces in the northern part of the United States. In this year also Agassiz published, in conjunction with Prof. A. A. Gould, a small educational treatise entitled 'Principles of Geology,' which is of importance as containing a clear statement of the opinions entertained by him on general questions of natural history.

In 1852 Agassiz accepted the Professorship of Comparative Anatomy in the Medical College of Charleston, which he retained for two winters, but then resigned it on account of his health.

About this time the great collections which he had brought together, and especially the immense number of marine animals which formed the fruit of his dredging excursions, led him to the idea of attaching a good museum to the Cambridge School. By his personal exertions and influence he was speedily enabled to found a magnificent museum, which promises to become the finest in the world. From 1855 Agassiz was almost constantly engaged in the determination of his accumulation of specimens and their arrangement in his new museum, and at this time also he formed the plan of publishing a series of descriptive papers, founded upon the objects in the museum, under the title of 'Contributions to the Natural History of the United States.' As originally projected, this work was to have been in ten volumes; but only two volumes have appeared. The 'Essay on Classification,' which formed part of the first volume, has been reprinted in a separate form, and has had considerable influence on the study of Natural History.

In course of time the incessant labour to which he subjected himself began to tell upon the health of Agassiz; and in 1867, by the aid of Mr. Thayer, he was enabled to take a holiday in the valley of the Amazon. But even then it was a holiday of hard work; and its results were published in 1868, under the title of a

‘Journey in Brazil.’ Other important works published by him in America are his ‘Essay on the Study of Natural History,’ and his ‘Comparative Embryology.’

In his general views on Natural History, Agassiz was strongly opposed to the doctrine of the origin of species by evolution from preexisting forms. In all his writings he maintained that species were produced independently of one another. At the very close of his life he was engaged in a discussion of this question, a first paper by him on “Evolution and Permanence of Types” having appeared in the *Atlantic Monthly* for December last.

During his residence of twenty-seven years in the United States, Agassiz devoted himself heart and soul to his professorial duties; and as he was one who possessed in an eminent degree the faculty of communicating his own enthusiasm to those with whom he was brought into contact, we can understand the great influence and popularity which he attained, although this cannot diminish our sense of the vast energy and activity which enabled him in so short a time to create a school of naturalists much of whose work is fully equal to that produced by their *confrères* on this side of the water. There seems to be no question that the rapid progress of natural history in the United States is due mainly to the teaching and influence of Agassiz.

The evident failing of Agassiz’s physical powers had for some years given much anxiety to his friends; and he was earnestly pressed by them to abstain from some, at least, of the labours which he laid upon himself. He lectured to his students on the 5th December, and was at work in the Museum of Comparative Zoology on the following day, when a sense of extreme debility came over him and compelled him to desist from his work. He was able, however, to reach his own house unaided; but when there he was compelled to take to his bed, which he never afterwards left. He died in the night of the 14th December last, the cause of death being “nervous prostration, resulting in an attack of paralysis, affecting the respiratory muscles, and especially the pharynx.”

Agassiz received numerous marks of recognition from scientific and other public bodies. He was LL.D. of Edinburgh and Dublin. He received the Balance of the Wollaston Donation-fund in 1834, and the Wollaston Medal in 1836; he was elected a Foreign Member of the Royal Society in 1838, of this Society in 1841, and of the Linnean Society in 1844; and in 1861 he received the Copley Medal from the Royal Society.

One by one the names which have always been associated with the early progress of geological science are dropping out of our list. Two years ago it was the loss of Murchison that we had to deplore last year Adam Sedgwick had just departed from among us; and we have now to record the loss of one whose name has for many years been intimately associated with theirs,

PHILIPPE-EDOUARD POULLETIER DE VERNEUIL, who died on the 29th of last May. M. de Verneuil was born in Paris, of a good family, on the 13th February 1805, and was consequently sixty-eight years old at the time of his death. His parents destined him to the profession of the bar; and with this prospect before him he attained the age of twenty-five, when the revolution of 1830 changed his prospects and induced him to seek some other sphere of activity. Being independent in his circumstances, his choice was not hampered by considerations of profit to be earned; and his attention having been attracted to the great advances lately made in geology, he attended the lectures of M. Elie de Beaumont, who was then assiduously promulgating the new views. From that period, all his time, energy, and fortune were devoted to the advancement of geology and palæontology.

While De Verneuil was studying the theory of geology under Elie de Beaumont in Paris, the two great English geologists with whose names his own was destined to be associated, were engaged in those investigations of the ancient rocks of Wales which have rendered that country classic ground. The grand results obtained by Sedgwick and Murchison in the grouping of those vast masses of old rocks which had previously been confounded under the common name of "Transition Formations" seem to have produced a lively impression on Elie de Beaumont's pupil; and we find him making his first geological journey for the purpose of examining the field of their labours. As is so frequently the case, the direction given to his mind by his first practical study of geological phenomena influenced his course through life; and it is almost exclusively as the geologist and palæontologist of the older palæozoic rocks that M. de Verneuil is known to us.

After his return from Wales De Verneuil did not remain long in France. His passion for geological travel drove him away into the East; he travelled down the Danube into Turkey, whence, having met with some congenial companions, he proceeded through Moldavia and Bessarabia to Odessa and the Crimea, and then to the

frontiers of Circassia, returning to Western Europe by way of the Bosphorus. The result of this journey was a memoir on the geology of the Crimea, published in the *Memoirs of the Geological Society of France*. The fossils brought from the Crimea by De Verneuil were determined and described for him by M. Dēshayes, who subsequently gave him a series of instructions in palæontology, a study in which he made great progress, so that in 1838 he was enabled to make an independent investigation of the Devonian rocks and fossils of the Bas-Boulonnais. In 1839, when Sedgwick and Murchison undertook their examination of the older Palæozoic rocks of the Rhenish provinces and Belgium, for the purpose of comparison with formations of the same age in Britain, De Verneuil was invited to accompany them; and in 1841, in conjunction with M. d'Archiac, he published in our 'Transactions' (2nd series, vol. vi.) descriptions of the fossils collected by him in those countries, both in this and other visits. This memoir is preceded by a general view of the faunas of the Palæozoic formations; and appended to it is a table of the organic remains which up to that time had been met with in the Devonian rocks of Europe.

The experience of this investigation demonstrated clearly the extreme importance, if not the absolute necessity, of palæontological knowledge in determining the geological structure of a country. Accordingly in 1840, when Sir Roderick Murchison commenced his geological examination of the Russian empire, he again requested M. de Verneuil to accompany him, and added to the party a second palæontologist in the person of Count Keyserling. Their survey of Russia occupied only three summers (1840-42); and during this period they explored a surface including nearly half the continent of Europe. The results of their researches were given to the world in 1845 in the well-known magnificent work on Russia and the Ural Mountains, the second volume of which, containing the palæontological evidence, was almost entirely the work of M. de Verneuil.

At this time the writings of American geologists had made known the enormous development of the older stratified rocks in North America. Exercising what was certainly, in the state of science at that period, a wise discretion, American writers had abstained from attempting to correlate the strata observed in their country with those which had been distinguished by names on this side of the Atlantic, and had grouped the American rocks in accordance with their local occurrence. Such a state of things could not but

excite the liveliest interest in so devoted a student of Palæozoic geology as M. de Verneuil; and in the spring of 1846, immediately after the publication of the great work on Russia, he took his departure for the United States, in order to study on the spot the vast series of Palæozoic rocks intervening between the earliest beds containing traces of life and the great Carboniferous formation, and if possible, to establish the correlations of these deposits with those of approximately the same age in Europe. By a careful examination of the fossils contained in local collections and of those which he obtained for himself from the rocks, De Verneuil succeeded in demonstrating that even in regions so wide apart as Europe and America, the first manifestations of life are very similar, and that the same types are successively developed through the whole series of Palæozoic strata, thus establishing a parallelism between the Palæozoic rocks of the two regions, which, although it may have received some slight modifications in the subsequent progress of our science, must still be regarded as a fundamental geological fact. The memoir containing the results of this investigation appeared in the 'Bulletin' of the Geological Society of France (ser. 2, vol. iv. 1847).

After his return from America, M. de Verneuil turned his attention to a region nearer home, and from 1849 to 1862 travelled repeatedly into Spain with the view of investigating the geology of that country. Up to this time Spain was almost unexplored; and De Verneuil was further induced to examine its geology and palæontology on account of a curious hypothesis entertained by De Blainville, that whilst the succession of strata and of the faunas which characterize them might be regarded as well established for the northern part of the continents of Europe and America, it was otherwise in the south, so that in Spain, and especially in its southern part, the order of succession of fossil species might be expected to be reversed, or at least considerably modified. In his numerous journeys into Spain, sometimes alone, sometimes accompanied by younger geologists, among whom was M. Edouard Collomb, De Verneuil carefully studied the geological structure of the country and collected great numbers of fossils, the examination of which showed that Spain furnished no exception to the ordinary laws of palæontological distribution. The results of these investigations appeared in the form of a Geological Map of Spain, and of numerous memoirs published in the 'Bulletins' of the Geological Society of France, which, indeed, still contain nearly all the information we possess on Spanish geology.

M. de Verneuil's activity was brought to a premature close by a gradually increasing defect of vision, which in the last years of his life threatened him with total blindness. But the work he had already done sufficed to obtain for him many marks of honourable recognition. In 1844 he was elected a Foreign Member of this Society; and in 1853 the Wollaston Medal was struck in duplicate for presentation to MM. d'Archiac and de Verneuil, for their numerous contributions to geology and especially for their joint paper "On the Fossils of the Older Palæozoic Rocks of the Rhenish Provinces." In 1854 he was elected a Member of the Academy of Sciences, and in 1860 a Foreign Member of the Royal Society.

In his personal character M. de Verneuil was distinguished for cheerfulness, simplicity, and kindness. He took an interest in all branches of knowledge. His large collections of fossils were always placed freely at the service of geologists and palæontologists who wished to examine them; and in order that they may still serve the same purpose, he has bequeathed them to the School of Mines in Paris.

The distinguished palæontologist AUGUST EMIL VON REUSS, who was elected a Foreign Correspondent of this Society in 1866, died at Vienna on the 26th of November, 1873, in the sixty-fourth year of his age. He was born at Bilin in Bohemia on the 8th July, 1811, received his early education at the Gymnasium of Prague, and afterwards studied medicine at the University of that city. On taking his degree in 1834 he was offered a position in the Ophthalmic Hospital at Prague; but the state of his health rendering it necessary for him to reside in the country, he established himself in medical practice in his native place, where he remained for fifteen years.

During his residence at Bilin Reuss commenced the study of mineralogy and geology; and as early as 1837 he brought before the Meeting of the Naturalists' Association at Prague an account of his investigations upon the geology of the neighbourhood of Bilin. Encouraged by the good reception accorded to his first efforts, he then gave a wider scope to his researches, and undertook an investigation of the Secondary Geology of Bohemia, the results of which he gave to the world in 1840 and 1844 under the title of 'Geognostische Skizzen aus Böhmen.' His most important work at this period was his 'Beschreibung der böhmischen Kreideversteinerungen,' published in 1846, which contains a detailed monograph of

the Cretaceous fossils of Bohemia, illustrated by an Atlas of 51 quarto plates.

In the year 1849, having become devoted to palæontological studies, Reuss gave up his medical practice and accepted the position of Ordinary Professor of Mineralogy in the University of Prague.

He was the first lecturer on geology in Prague; he established a large mineralogical collection there; and during his residence there he produced some of his most valuable works, including his '*Monographie der Gosau-Versteinerungen*,' his '*Beiträge zur Kenntniss fossiler Krabben*,' and his '*Versuch eines Systems der Foraminiferen*.' In 1863, on the death of Professor Zippe, Reuss was nominated to succeed him in the Chair of Mineralogy in the University of Vienna; and this position he held until his death.

The great merits of Professor Reuss gained him many honourable distinctions. He held honorary degrees of the Universities of Breslau and Vienna; he was twice Dean of the Philosophical Faculty, and once Rector of the University of Prague. In 1848, immediately after its foundation, the Academy of Sciences in Vienna elected him one of its members; in 1854 he received from the Emperor of Austria the Cross of the Order of Francis-Joseph, and at a later period the Iron Crown of the Third Class.

The scientific activity of Prof. Reuss was very great; the list of his scattered papers would include considerably more than 100 titles. His original work was chiefly in the domain of palæontology; and here he devoted his attention especially to the lower forms of life, such as the Entomostraca, Corals, and Foraminifera.

GUSTAV ROSE, who was elected a Foreign Member of this Society in 1840, belonged to the third generation of a family of chemists. His grandfather, Valentin Rose, was a distinguished pharmacist of the earlier part of the eighteenth century. His father, Valentin Rose the younger, who succeeded the older Valentin, and became Assessor in the "*Obercollegium Medicum*" of Berlin, died in 1807, leaving four sons to be brought up by his widow. Gustav Rose, the youngest of these sons, was born in Berlin on the 18th March, 1798. His brothers served in the Prussian army during the last wars against Napoleon; but the young Gustav, who was only seventeen years old at the time of the Battle of Waterloo, was not present in that engagement, although he was then under arms, and subsequently marched from Berlin to Orleans with a part of the army of occupation.

In 1816 Gustav Rose commenced his career as a student of mining at Königshütte in Silesia; but during his recovery from an attack of inflammation of the lungs he took to purely scientific studies, and finally, induced by the example and perhaps the advice of his elder brother Heinrich, he determined to devote himself entirely to such pursuits. He commenced his studies in Berlin in the year 1816, and in 1820 took his degree of Doctor of Philosophy in that University. In 1821 he visited Stockholm, and joined his brother Heinrich in studying under the great Swedish chemist Berzelius, and on his return to Berlin commenced his career as a "Privatdocent" in 1823. He became Extraordinary Professor of Mineralogy in the University of Berlin in 1826, and Ordinary Professor in 1839, and in 1856 (on the death of Weiss) was appointed Director of the Royal Mineralogical Museum. He was elected a Member of the Academy of Sciences at Berlin in 1834. His earliest published paper, "Ueber den Feldspat, Albite, Labrador und Anorthite" appeared in Gilbert's 'Annalen der Physik' in 1823. His subsequent writings were very numerous: the list published by the Royal Society gives the titles of 120 papers produced by him up to the year 1863; and until very nearly the end of his life nearly every part of the "Monatsbericht" of the Academy of Berlin contained contributions from his pen. In these papers he covers, says Prof. vom Rath "all departments of mineralogy—the form and combinations of crystals, physics as applied to crystallized substances, the chemical constitution of minerals, and their artificial formation. He was a great master in the art of crystallographic drawing. The science of the association of minerals in rocks, petrography, originated with him; and he was the first to teach us the method of studying rocks by means of their microscopic sections mounted on glass slides, in which minerals invisible to the naked eye are disclosed." According to the same authority, Gustav Rose was the first in Germany to use the reflecting goniometer for the exact measurement of the angles of crystals; and he took an important part in the researches which led Mitscherlich to the discovery of isomorphism. His researches on meteorites were very extensive and important; and their results are embodied in the valuable treatise entitled 'Beschreibung und Eintheilung der Meteoriten' published at Berlin in 1863. Of his other works the most important are:—the 'Elemente der Krystallographie,' of which two editions have been published; the 'Krystallochemische Mineralsystem,' published at Leipzig in 1852; and his memoir 'Ueber das Krystallisationssystem des

Quarzes,' which appeared at Berlin in 1846, and in which he showed how much could be done by the investigation even of a mineral already so well known.

For many years Gustav Rose travelled extensively and visited most parts of Europe—Scandinavia, England and Scotland, Italy and Sicily, France and Austria. In 1829 he accompanied Humboldt and Ehrenberg on their expedition into Central Asia, and explored with them the Ural and Altai Mountains and the shores of the Caspian Sea. The report on the mineralogical and geognostic results of this journey, which extended nearly to the borders of China and first made known the mineral resources of the Russian Empire in Asia, was prepared by Gustav Rose, and formed part of the great work entitled '*Reise nach dem Ural, dem Altai und dem Kaspischen Meere*' published at Berlin between 1837 and 1842, in which the observations of the three distinguished travellers were recorded.

Throughout a long life Gustav Rose devoted himself most thoroughly to his mineralogical investigations, on which he brought to bear great acuteness and a grasp of mind which enabled him to take into consideration all the physical characters of the mineral he was studying. In his personal relations he was cheerful, straightforward, and charitable in his judgments of others, and as a natural consequence was regarded with affectionate respect by all who knew him. As a teacher he threw all his energy into his work; and he continued to discharge this portion of his duties until the very end of his life. On the 11th July, 1873, he delivered his last lecture; in the evening of that day he was attacked by pneumonia; and on the 15th July he died.

KARL FRIEDRICH NAUMANN, the son of Johann Gottlieb Naumann, a distinguished musician and composer, was born in Dresden on May 30, 1797. He was educated at Pforta, and from 1816 studied under Werner at Freiberg. After Werner's death he devoted two years and a half to the study of Natural History at Leipzig and Jena. At the latter place he took his degree. In 1823 we find him occupying the position of "*Privatdozent*" in Jena, and in 1824 in Leipzig. He afterwards attended the lectures of Mohs at Freiberg; and on the departure of Mohs, in 1826, to fill the Chair of Mineralogy at Vienna, Naumann succeeded him as Professor of Crystallography. He was also appointed Proctor in the Mining Academy at Freiberg. These offices he retained until 1835, when

he became Professor of Geognosy in the same institution, and was charged with the preparation of the geological map of Saxony. In 1842 he was appointed Ordinary Professor of Mineralogy and Geognosy in the University of Leipzig, in which position he remained until his death, which took place, after a short illness, on the 26th November last. Professor Naumann was elected a Foreign Member of this Society in 1855; and received the Wollaston Medal in 1868.

Professor Naumann's papers and other writings on subjects connected with geology are very numerous. In 1821 and 1822 he travelled in Norway; and the results of his observations were published in 1824 under the title of 'Beiträge zur Kenntniss Norwegens' (Leipzig, 2 vols.). In 1828 he published in Berlin a 'Lehrbuch der Mineralogie,' which has passed through many editions, and has always been one of the chief textbooks of that science in Germany. His 'Lehrbuch der reinen und angewandten Krystallographie,' a most valuable book, appeared at Leipzig, in 2 vols., in 1830; but his crowning work is the great 'Lehrbuch der Geognosie,' published at Leipzig from 1850 to 1853, the most elaborate and comprehensive textbook of geology that has appeared in any language. A second edition of this work was published in two large 8vo volumes in 1858-62.

DR. JOHANN JACOB KAUP, elected a Foreign Correspondent of this Society in 1863 and a Foreign Member in 1871, was born at Darmstadt on the 20th April, 1803. With a few occasional absences he seems to have resided all his life in his native city, where he received in 1840 the appointment of Inspector of the Grand-Ducal Cabinet of Natural History, which he retained until his death on the 4th July, 1873.

Dr. Kaup was a writer on various departments of Natural History. His earlier papers were contributed to Oken's 'Isis,' his later ones chiefly to Wiegmann's 'Archiv für Naturgeschichte.' His description of the remarkable mammal, *Dinotherium giganteum*, from the Miocene of Eppelsheim, although not the most important of his palæontological writings, is probably that which has rendered his name most widely known; it was published in Oken's 'Isis' as long ago as 1829. In Palæontology he devoted his attention chiefly to Vertebrate animals, and, among these, especially to the Mammalia; his largest work relates to this class, namely the 'Description d'Ossements fossiles de Mammifères inconnus jusqu'à

présent, qui se trouvent au Musée grand-ducal de Darmstadt.' This appeared in 4to, with a folio atlas of plates, at Darmstadt, between 1832 and 1841.

Dr. Kaup's works on recent zoology relate to many groups of animals, but especially to Fishes and Insects. He was one of those naturalists who hold fast to the faith of their youth; and, to the end of his life, his views on the systematic representation of nature were allied to those of the so-called philosophic school of naturalists of the last generation, represented in this country by McLeay, Vigors, Horsfield, and Swainson. Dr. Kaup's last published work was a Monograph of the Coleopterous family Passalidæ, in which he proposed an arrangement of those insects in accordance with and illustrative of his peculiar doctrine of types.

I will not waste the time of the Society by any lengthened apology for the imperfect manner in which I have been able to discharge the duties of this Chair. When the Council did me the honour of selecting me as President, they were aware of the special circumstances which, apart from and besides my very limited scientific knowledge, would necessarily prevent me from devoting any adequate time to the business of the Society. I should therefore not have been disposed to say any more on the subject, were it not that the duty of drawing up something in the shape of an Address at the close of my period of office brings again very forcibly and very disagreeably before me the deficiencies under which I labour. The duties of my political office have during the last year been exceptionally heavy: and I am obliged to confess that I have been wholly unable to follow even in the most hasty manner the course of geological discussion or discovery during that period. I can do no more, therefore, than offer with great diffidence to the Society a few general observations on the position of our science with reference to some of the larger problems with which it deals, and which it aspires to solve.

I hope it will not be thought presumptuous on my part if I direct these observations rather to the shortcomings than to the triumphs of geology, rather to the questions which remain unanswered than to those which have received a satisfactory reply. On the last occasion on which I had the happiness of seeing Mrs. Somerville—a woman whose charm of manner, and whose simplicity of character were as remarkable as her extraordinary

mental gifts—I was greatly struck with the impressed and impressive tone in which she exclaimed, at the close of some discussion—half to herself, half to us who were around her—“Ah, how little it is we know!” I wish to make this humble and half-mournful exclamation of a most powerful and aspiring mind the text of my Address to-day, and, if possible, to give it a definite and practical application to the present condition and aspect of geological inquiry.

There is a region in which geology passes into cosmogony, and questions connected with the origin and history of our globe merge in the more general questions which arise on the history and condition of the worlds around us. Historically we know that cosmogonies have preceded science, and have belonged exclusively to imagination or belief. With that impatience to reach ultimate results which after all is one of the noblest characteristics of the human intellect, and when placed under the discipline of method becomes the sustaining spirit of research, men rushed hotly to conclusions as to the processes of nature which were not based upon observation, and either took no account of existing agencies, or formed no adequate estimate of their power when multiplied into time. We cannot be too grateful to the fathers of all true geological science who brought this tendency under correction, and trained men to see and, in some degree, to measure the effects of those energies which, because they are familiar, are comparatively unobserved. To no man has the science of geology been more indebted in this particular than to our distinguished associate Sir Charles Lyell, who in upholding the adequacy of ordinary causes has kept the great argument of Hutton and of Playfair well abreast with the progress of discovery, and has defended it with a wealth of knowledge and a force of illustration which the science of their time did not enable them to wield.

Nothing, indeed, can detract from the service which has been rendered by this argument in the stimulus it has given to minute and careful observation of the ordinary operations of nature. On the contrary, every thing has tended and is still tending to confirm its truth and to extend its application. But in this very extension there lies a new and better understanding of all that it involves. Every thing that we see is the result of “ordinary causes:” yes, but what are they? Other sciences are coming into more intimate connexion with our own, and are widening our horizon as to the causes and forces which are to be reckoned ordinary in the operations

of nature. Our planet is only one of many; and our whole solar system is only one of many more. Suddenly and quite recently we have acquired the means of knowing something of their physical constitution; and we awake to conceptions which are altogether new of the tremendous phenomena which are now common in the universe, and of the stages of condition through which much planetary matter is now passing, and perhaps all has passed. Some of the bodies which we see around us are pure gas, some are swarms of meteoric particles, many appear to be in different stages of condensation; and our sun is in a state of such fierce combustion that the tongues of flame which it emits are seven or eight times the whole diameter of the globe. It is not possible that we should know these things without acquiring a new idea of the operations which are of ordinary occurrence in nature. It casts no doubt on the uniformity of causation, but it immensely affects our estimate of what that causation may include. The enlightened advocates of that doctrine have always argued that it does not exclude catastrophes, because catastrophes of this and the other kind have been common on the earth. And so it will be with the larger knowledge we have now acquired of the agencies at work in shaping and forming the bodies of which our world is one. Only our idea of the nature or extent of the catastrophes which are or may be included in the "ordinary" course of nature, will no longer be formed on the eruptions of Hecla, or on the earthquakes of the Andes. Forces or activities more violent than the wildest imagination has conceived in theories which have long been abandoned as visionary and absurd, must now be reckoned as clearly within the range of ordinary causation as those of the river and the wave.

The truth is, that uniformity of causation, when it is understood in that larger sense in which alone it is true, ceases to have many of the applications which have been commonly supposed. In philosophy we see how the Doctrine of Experience has been compelled to pass from the experience of the individual and to take in the inherited experience of the family, of the race, of the species, and finally of the organism, whatever its nature or history may involve. But when thus understood, the Doctrine of Experience comes to cover and include most of the old doctrine of Innate Ideas. And so the doctrine of the Uniformity of Causation must be understood as embracing all the vast unknown cycles of cosmic change. But when thus understood, it comes to cover and include all that

was ever suggested in the theories which assumed more intense and violent operations than are consistent with the present aspects of nature. No theoretical cause can be rejected merely because it is not working here and now. Enough if it be among the causes which are working now in other worlds, or may probably have been working here at other times. Our belief in a Glacial epoch extending over regions of the earth now enjoying a temperate climate involves a belief in the greater intensity of former causation, as compared with existing conditions there. The particular form of that theory which assumes the existence of an ice cap, involves a belief in the greater intensity of former causation as compared with existing conditions in any portion of the globe. But if this licence be given (and I do not see how it can be denied) to those whose minds are set on the atmospheric agencies of changes, neither can similar licence be denied to those whose regards are fixed on the operations of subterranean force. Both are equally real. Both are equally included in the vast and various uniformities of nature. Both are capable of action in an infinite variety of degrees. It is no more in violation of the uniformity of causation to suppose former upheavals and depressions with which no existing subterranean movements can compare, than to assume the former operation of denuding agencies enormously more prevalent and more powerful than are working now.

Then there is another fact which tends to reconcile the idea of energies formerly more intense than now with the general truth of uniformity in causation; and that is the prevalence of a law of periodicity in so many of the operations of nature. All the ordinary atmospheric phenomena of the seasons are of course regulated by this law within the limited cycles of time which are measured by our days and years. And so it is quite according to the analogies of nature that there should be longer cycles bringing with them changes of corresponding magnitude and extent. These are the oscillations, as Mrs. Somerville finely says, "of that great pendulum of eternity which beats centuries as ours beats seconds." Accordingly some geologists imagine that they have discovered traces of recurring Glacial epochs in the lapse of ages which the rocks record. If this surmise be true, the Glacial epochs which we know as such, and which, some geologists believe, carried the rigours of an Arctic climate down even to the equatorial latitudes, may have been nothing more than a long recurring winter of many centuries—a winter which had been before and which will come

again—just as in like manner it may have been the glorious summers of another epoch of as prolonged duration which carried the splendours of Carboniferous and Miocene vegetation close up to the borders of the Pole.

I do not know that any similar periodicity has ever been suspected in the exertions of subterranean force ; but no such supposition is required in order to reconcile a belief in its former intenser action with that other more general belief in the uniformities of nature, which, when rightly understood, is among the axioms of science. We have only to open still a little wider the eyelids of our understanding, and to take in yet another somewhat wider circle of the immensities of space and time. The course of things upon this globe since it assumed any thing approaching to its present condition and form, may have been the course of a steady and continuous decline in the energies of subterranean force, which are probably nothing else than the energies of heat ; and yet this passage from a greater to a less intensity of action may be a part, and a regular part, of the ordinary operations of nature—a stage, and a very common stage, among the millions of worlds around us, in that continuous preparation for the abodes of life, which may be the destiny of those worlds, as it certainly has been the destiny of our own.

But although these are considerations which restore to geological speculation much of the freedom which has sometimes been denied to it under an interpretation of the law of uniformity which cannot be sustained, it is equally true that the right interpretation of that law does impose limitations which are constantly transgressed. That which is really uniform in nature is the effect of a given cause under the same conditions—the operation, for example, of a given force applied in the same way and upon the same material. But this complete identity of conditions is essential to the uniformity asserted ; and when it does not prevail, no such uniformity exists. On the other hand there are certain effects which a given cause is not competent to produce ; and under no conditions can those effects be the result of that cause. This negative aspect of the uniformities of nature is, perhaps, of more practical importance than the positive aspect ; for it is probably more easy to recognize and establish some limited number of things which cannot be done by a particular agency under any conditions than to enumerate the infinite variety of things which may possibly be done by it under conditions which are practically inexhaustible. It is easier to fix on a few things certainly excluded, than to follow even a small fraction of all that

may possibly be included. And yet even this selection and identification of a few impossibilities may require all the care of the most sagacious observation, and all the resource of the most difficult mathematical calculation. It is certain that many theories have lived long and do still live solely upon assumed possibilities, which are really among the impossibilities of nature.

It is thus that many of the effects which have often been ascribed to earthquake-waves were pronounced to be physically impossible in a most interesting paper by Mr. Mallet, and in the discussion upon it during the Session of 1872. A considerable number of us who were present on that occasion were not able to follow the calculations by which that impossibility was established; and where this is the case there must always be some doubt and hesitation in the general acceptance of such conclusions.

It would be well, however, if there were more of this method of rigorous inquiry in our speculations on geological causation. This is the only real method in which the great doctrine of the uniformity of causation can be successfully applied. Thus, if it were established that the hardened and consolidated crust of the earth, when subjected to lateral pressure from subsidence, must always fold and can never break—or if it could be established that such fractures as do occur must be in the form of slippings or slidings, such as are common in ordinary faults, and that “gaping faults” are not to be admitted as possible—or if, on the contrary, it were established that with given degrees of tension, and under given conditions, such fractures might or, it may be, must arise, much would be done either way to direct us in the interpretation of geological phenomena. In like manner the question whether the physical condition of ice, and the laws of motion and of resistance to pressure, which govern its behaviour, do or do not render it incapable of doing certain work, is a question which must be discussed and decided on similar principles before we can have any confidence in the theories which assume its agency in the cutting of existing surfaces. Glacial conditions of an intensity and of a prevalence much greater than any which now exist may be quite possible, quite within the ordinary course of nature; and nevertheless the work assigned to them may be quite impossible, because absolutely excluded by the uniformities of causation.

In a former paper I explained the grounds on which it appears to me that work has been assigned to glaciers which they are incapable of performing. But it is impossible not to recognize the fact that the competency of this agency to produce some of the most striking

and some of the most charming features of physical geography is maintained by geologists of the widest experience, and of the keenest observation.

This question, I am afraid, cannot be settled until we know more accurately and more completely the laws which would govern the movements of ice in mass—not merely when hanging on a mountain-slope, but when resting upon and moving along level or ascending surfaces.

I venture to think that this is a subject on which we are still very imperfectly informed, and to which it is most important that the researches of physicists should be carefully directed.

It is not, however, the conditions of glacial action alone respecting which we have much to learn. This is only one, and not the most continuous one, of those agencies of change which, together with subterranean force, have been the immediate causes of geological structure. And although the mere dynamics of aqueous action, whether fluvial or marine, are probably much more accurately known, I am disposed to think that the conditions under which denudation has been effected by water present some of the most puzzling problems in geology. That denudation and deposition are inseparable correlatives, that the quantity of material accumulated in one place is a measure of the mass of matter which must have been removed from another—this is, indeed, one of those great general conceptions, the very breadth and certainty of which may readily deceive us as to the real amount of knowledge which it conveys; for within the limits of this almost abstract truth there is room for an infinite variety of error as to the methods of operation which have been pursued in nature. The work which is done by denuding agencies is the product of two factors—first the power of those agencies in themselves, and secondly the conditions of mechanical advantage under which solid material has been exposed to that power. And of these two factors the last may be the most important, as it certainly is the most obscure. Signal illustrations of this have been left us by the Glacial epoch. The time which has elapsed since its close must have been very great, long enough certainly to effect in many places physical changes of no small amount. Yet in other places the amount of denudation which has been effected since the close of the Glacial epoch by all the ordinary meteoric agencies of erosion has been so infinitesimally small that the finest and minutest striæ made by ice loaded with sand upon rock surfaces appear to be as clear and sharp as if they had been the work of yesterday.

Sometimes this is due to peculiarities of chemical composition which resist chemical action; but more often it is due to the mechanical position in which the bedding has been and still is exposed. Agencies which are almost powerless against a rock when it is exposed in one position, with reference to its bedding, will attack it with rapid success if that position should be changed. But such changes are the special work of subterranean force. In the upheaval and subsidence of the surface, in the bendings and contortions of strata which such movements must involve, we must look not only for the directions in which erosion must take place, but for the mechanical conditions which determine its rapidity and its amount. I observe that in the increasing number of papers which are devoted to the elucidation of the phenomena of the Glacial epoch, there is an increasing recognition of repeated and recurring elevations and depressions of the land. In the course of one of these it seems now to be generally admitted that our existing mountains have been submerged to the extent of at least 2000 feet. This is established by more than one kind of proof—of which perhaps the position of gravelly deposits, including shells, is the most certain and convincing. But living, as I do, in a mountain-district where the peculiar position and distribution of travelled boulders and of perched blocks affords, in my opinion, evidence of this fact which is quite as strong, I believe it to be one among the most certain conclusions of our science. Yet I think it may be said with truth that we are almost entirely ignorant of the causes of such a movement, as well as of many of the conditions of its progress. Nor can I think that its consequences and effects have been as yet identified with any approach to completeness or consistency. How far did the area of submersion extend? Is it possible to suppose that it was limited to these islands? What part had it in the great work of denudation? What correlative part had it in the distribution of gravels which are so often assigned exclusively to fluvial action? Does not the comparative absence and, often, the fragmentary character of shelly remains indicate action so sudden and tumultuous as to have excluded the quiet life of a molluscan fauna? And then if the movement was not, as regards geological time, slow and consistent with the continuity of life, but in the same sense sudden and destructive, what may not have been the effects of such a movement on the outlines of physical geology? There are facts which would seem to indicate that the contour of the surfaces before and during submersion were very much what they are now. Yet it is impossible that those sur-

faces should have been submitted to ocean-currents loaded with floating ice without having been greatly modified; nor, as it seems to me, is it easy to account for the subsequent removal of the masses of loose material which must have accumulated during this great submission to the sea. Even the most superficial facts bearing on the configuration of the surface before and after the Glacial epoch are still very variously read and interpreted. In the very last number of our Journal I observe two papers on the glaciation of the Hebrides. One of these describes the movement of the abrading ice as having distinctly been from S.E. to N.W. The other as confidently decides that the movement was precisely the reverse, from N.W. to S.E. These different verdicts on this one point are connected with widely different conclusions with respect to the history of the surface. One of them would indicate an ancient land to the N.W. now submerged and lost; the other would indicate only a much greater elevation of the existing mountains of the N.W. coast of Scotland.

And if we are as yet so imperfectly acquainted with the circumstances and effects of a subterranean movement which has unquestionably been among the last great causes of physical change, we must have still more to learn of those more ancient exertions of subterranean force which have been nevertheless among the ordinary and recurring operations of nature. The complete and absolute breaking up and reconstruction of terrestrial surfaces which becomes a familiar abstract conception to the geologist, demands the operation of causes amongst which denudation has been the servant and not the master.

It is impossible to take one's stand on certain points of the earth's surface where these great changes are visible to the eye, as, for example, on every high peak overlooking the Hebrides, without seeing that we have at our feet the remains of ancient lands, which have not been merely rubbed down and washed away, but which have first been broken into fragments by the giving way, as it were, of the very foundations on which they stood.

In connexion with this subject there is one question on which I am disposed to entertain a doubt whether we are not apt to proceed implicitly on an assumption which is really erroneous. There is no principle of physical geology which it was more difficult to bring home to vulgar apprehension than the principle that, as regards apparent changes of level in sea and land, the standard of constancy is not in the land, but in the sea. It seemed hard to understand that

an element so proverbially unstable as water should be regarded as the fixed standard of comparison, and that the solid rocks, "the everlasting hills," should be looked upon as the real seat of instability and movement. A conclusion so contrary to first impressions was not at first easily received. On the other hand I am not sure that it is not now accepted too absolutely, and without the qualifications which limit its applicability and its truth. The level of the ocean is constant, not absolutely, but with reference to itself, that is with reference to all other parts of the same aqueous surface. It cannot permanently rise or fall on any one coast-line without an equal and similar movement all round the world. But no such movement has taken place, or at least none such is known to have taken place, in the human period; consequently we are apt tacitly to assume that it is not in question. All the movements of which we have any evidence as going on now, and perhaps most of those of which we have any clear evidence in recent geological time, have been local in the area affected. Any and every partial and apparent rising of the sea, any and every submersion of a particular area of the land must be due to movements in that material which is, indeed, the most solid, but which also on that account has no necessary persistency of level. It becomes therefore a most natural assumption, on which we proceed almost unconsciously, that all the lands we now see have been raised by upheaval from the level of the existing ocean. But this does not follow: there is no absolute constancy in the level of the ocean; that is to say, there is no fixedness in the position it occupies with reference to its distance from the centre of the earth. Movements affecting its level with reference to this radial distance, and of course affecting it simultaneously all round the globe, are not only conceivable, but are consistent with the physical probability of shrinkage and great subsidences in the crust of the earth. This would cause the gathering of the waters into hollows and abyssal depths, and a general lowering from one level to another. I need not say that many of the appearances of upheaval would equally arise from such subsidence. The falling inwards and downwards of sedimentary beds from a higher level to a lower one, and the elevation of them from a lower level to a higher one, would present to the eye very much the same phenomena. And yet there might be, and probably would be, differences indicative of the one kind of movement rather than the other; I cannot help thinking that such indications do exist in almost all mountain-countries where geological structure is visible to the eye. My belief is that many

facts are more consistent with the effects of shrinkage and subsidence than with the effects of upheaval and elevation. Of course the one kind of movement constantly involves the other, movement upon an axis of tilting over one area being coincident with falling down over another. But my impression is that the floor of the ancient seas in which many of the older formations were deposited stood at a higher level than the floor of the existing seas, and that this general conception simplifies to a considerable extent the phenomena presented by the ruins and fragments of former lands, such as are presented, for example, in the Hebrides.

The mere general Huttonian conclusion that existing terrestrial forms are the result of the two great contending agencies of elevation and degradation is of course true; and the establishment of this conclusion was a great gain, and a great step at the time when it was established. But the whole problems of modern physical geology lie inside this general conclusion. How, and under what conditions the elevatory forces have worked, how slowly or with what degree of occasional suddenness,—whether nothing but mud-banks have ever been upheaved from the sea,—whether extensive fractures of land-material have ever been occasioned,—whether these, if they have occurred, have always been underground, or have somehow never appeared upon the surface,—all these are questions undetermined, and on which a great variety of answers would be equally compatible with the only general truth which has been yet established. And so with respect to the one great agency of degradation, water: in which of its many forms has it been most powerful? in rain, or frost, or snow, glacial ice, or in the vast mass or volume of the everlasting sea? and if the last, then, again, under what conditions of upheaval or of fracture, and of sudden subjection to its tumultuous action, or of nothing more than mere constant gnawing at comparatively fixed coast-lines has its vast work of removal been accomplished? All these are questions which there is nothing in the Huttonian theory to solve, and respecting which we only conceal our ignorance when we cover it under the formulæ which that theory supplies.

Accustomed, as I am, to look at the existing contours of a mountain-country, I never do so without feeling how hard it is really to account for them—how hard it is, I mean, to follow with any thing like distinctness of conception the nature and succession of the operations which have brought them to their present aspects. The long lines of precipice which can never, in their present posi-

tion, have been sea-cliffs,—the sudden “corries” which are entirely destitute of running water, and can never have been otherwise since the configuration of the country was, even in outline, like its configuration now,—the deep valleys which, during the same configuration, can never have been occupied by streams of any considerable power, and some of which are prolonged at great depths under the existing level of the sea,—the peaks now high in air, capped by remaining fragments of perfectly horizontal strata,—vast sheets of trap rising pyramidally into mountains, showing immense removals of the upper layers, and yet in positions where neither streams can have run, nor glaciers can have lain,—these and a thousand other appearances familiar to those who look for them present so many difficulties in conceiving, with any approach to distinctness, the methods of operation through which they have been brought about that it is impossible not to feel that we are still profoundly ignorant. In this, however, as well as in other departments of investigation the alternate prevalence of exclusive theories is one of the conditions of progress, a condition not certainly wanting in the present day. Physical geology now engrosses the attention and the ingenuity of not a few of our most distinguished Members; and during the time I have had the power of occupying this Chair Prof. Ramsay, Mr. Tiddeman, Mr. Bonney, Mr. J. C. Ward, Mr. Drew, and Mr. James Geikie have contributed remarkable papers to the elucidation of one or other of the problems involved. Quite recently a most valuable paper by Mr. Judd, on the Igneous Rocks of the Hebrides, points to a field of investigation on which I have long been satisfied the most important results may be attained. He who shall trace successfully the long series of changes through which the west coast of Scotland and its islands have come to assume the forms which they now present, will have done much to solve the obscurest problems of physical geology.

And now I pass to that other great department of our science which comprises the history of Organic Life upon the globe. Here also I desire to indicate the shortcomings of our knowledge and the failure of our discoveries as yet to solve the last and the highest problem of all. One great general conception has been; indeed, established—that life is as old as the existence of an ocean, that its types and forms have been perpetually dying out and have been as constantly renewed. Imperfect as the series confessedly is, the uniform tendency of discovery has been to fill up gaps, and more and more to represent this history of death and of renovation as

having been regular and continuous. And so the imagination fills up the vast intervals which still remain in blank, and concludes that, if only the record were complete, it would reveal the process and the steps. If only we could discover any considerable area of our earth's surface rich in life, and trace its history during some period long enough to comprise the disappearance of some species and the introduction of others, surely we should find out this great secret, and have visibly before us all the gradations of development,—a very natural conclusion, but, strange to say, one which never comes nearer verification, and is perpetually receding from the tests of proof. The record, indeed, of terrestrial surfaces and of terrestrial organisms is so imperfect as to represent mere shreds or patches. But this cannot be said of the fauna of the sea. There are such areas of the earth's surface of which the record appears to be complete, and is long enough to embrace considerable changes in the forms of life, including the introduction of new and well-marked species. Accordingly the links are all there; but somehow they are links which merely approach and never touch each other. The gaps are small and narrow; but somehow they are never bridged. Look at any good series of the Brachiopoda of the Secondary rocks; and to an unlearned eye nothing could be more perfect than the series, nothing more continuous than the gradations. But those who know them well know that the characters of every one of these species is constant from the moment it appears, and are never for a moment mingled with another. Take the case of the record presented by the remains of the Liassic sea; looking at the breadth of area over which it appears to have extended, looking at the uniformity of its deposits over the whole of that area, looking to the mineral character of the beds, looking to their undisturbed position, I know of no good reason to doubt that we have in them a tolerably complete history of life during the period of time, whatever that may have been, which was occupied by the accumulation of the Liassic beds. And that period was unquestionably long enough to witness the introduction of many new species, whilst, at the same time, it was not marked by any sweeping change, many species having lived undisturbed and unmodified throughout the whole. Moreover the most remarkable novelties which appear are in the highest types of the Mollusca. Who can solve the strange enigma presented by the successive changes which appear in the Cephalopoda of the Lias?—the definite and well-marked distinctions which suddenly appear, which are sometimes strictly confined to one bed in the series, and

which are again succeeded by others equally definite, and as strictly confined to that one period of time which is measured by the thickness of a particular sediment? I do not say, nor do I think, that these phenomena are inconsistent with the general idea of Evolution. On the contrary they may be embraced within it without any difficulty; but then the general idea of evolution is of a very elastic nature: the history, for example, of all human inventions is preeminently a history of evolution. All our tools, implements, and machines, all the patterns of our clothes, furniture and ornaments have been evolved. They are survivals. Would that we could always say that they have been survivals of the fittest! But in what medium is this evolution carried on? And what is the connexion between the developing agency and the product which is evolved? Certainly in these cases the medium in which the evolution takes place is mind and thought; and the connexion between it and the product is that of parentage, only in a metaphorical sense. Here, again, we find, as in former instances, that we are in danger of confounding a general idea unquestionably true, with one or more particular theories which embody that general idea in forms that may be full of error.

Possibly some Members of the Society may not have observed the curious passage upon this subject which occurs in Mr. G. Lewes's recent work entitled 'Problems of Life and Mind.' It represents geologists as the victims of an illusion equally when they search for "missing links," and when they account for the not finding them by the "imperfection of the record." Evolution, it is said, and the continuity of the animal series which it assumes, are an idea—a mental conception, and nothing more. It is like the idea of a typical form, of the archetypal skeleton, for example, which is an ideal interpretation, and not a physical reality; so that it would not be more absurd to seek for such a skeleton in the rocks than to look for a perfectly graduated series of forms as requisite for the proof of evolution. I am sure I need not in this company of practical geologists point out the fallacy of this method of dealing with one of the most interesting problems connected with our science. It is, indeed, perfectly true, as I have already indicated, that the general idea of evolution is entirely independent of any particular theory as to the means or the method by which that evolution has been brought about. The history of the steam-engine, from the earliest days when it appeared in its rudest form, to our own time when it has been brought to the highest degree of me-

chanical perfection, and adapted to a vast variety of work, is strictly a history of evolution. Nevertheless steam-engines were not born of each other. On the other hand it is equally true that if the evolution of organic forms has been effected in one particular manner, and in that alone, namely by ordinary generation, with no other changes than such minute variations as are always arising, and by these gradually accumulating, then this is a physical fact of which definite evidence must have existed, whether that evidence has been lost or not. The intermediate forms have existed or they have not. If they existed, they existed, not as the archetypal skeleton exists, as an abstract conception in the human mind, but as living, moving, breathing, and digesting creatures. Therefore our quest of them is founded on no illusion; and if we cannot find them, then some explanation of their absence is really necessary to the maintenance of our belief in that particular method of evolution which assumes them to have lived.

There is, however, one other explanation possible; and if I understand the passage in Mr. Lewes's work, it is an explanation which is there somewhat obscurely indicated. That explanation is simply this, that the intermediate forms which once existed do still in some cases exist—that they are not wanting, but are found. It may be quite possible to maintain, for example, that in the new species of Brachiopoda or of Cephalopoda which appear in the Lias and the Oolite we have a record of the new births which actually arose—that Terebratulæ and Ammonites of new patterns were suddenly born from the older forms, and from some unexplained cause bred and spread rapidly, remaining constant for a given period, until, in their turn again, they gave birth to forms differing from themselves by an equal interval. This is a conceivable supposition; and I am not sure that the differences between several species which have been preserved are not quite small enough to make it much more easily conceivable than is commonly supposed. Some of the species of Ammonite differ from each other in little beyond the pattern of the shell; and although each pattern is constant when once it appears, yet it is by no means impossible to suppose that it may have arisen quite suddenly, and simply as an unusual birth. It has been pointed out to me by Mr. Etheridge that there is one Ammonite in which an analogous change of pattern actually does take place at a particular stage of the creature's life, so that the inner whorls or convolutions of the shell are on one pattern, and the outer convolutions upon another. These two patterns, if they were not combined in the same

individual, but were always separate, would be sufficiently distinct to constitute two good species. This, no doubt, is a rare case; but it suggests the possibility of new laws of growth arising suddenly, and remaining constant in their products until some other similar change arises.

But this and every other theory as to the method in which the animal series has come to be produced is simply founded on our own absolute incapacity to conceive any other origin for organic forms than that with which we are familiar. This incapacity of our own may or may not be founded on a real impossibility in nature. But every one of such theories involves some law of change, in definite directions, of the nature of which as yet we are entirely ignorant. And in this law, whatever may have been the physical process through which it worked, is to be found the real secret of evolution. We cannot explain as it were mechanically results which absolutely demand higher methods of interpretation. "The truth is," as Agassiz wrote to me not many weeks before his death—"the truth is, that life has all the wealth of endowment of the most comprehensive mental manifestations, and none of the simplicity of physical phenomena." It is best, as it seems to me, to own that it is a subject on which we are as yet, at least, profoundly ignorant; and although I entirely disagree with Mr. Lewes that the search for transitional forms is futile and illusory, this, I think, may be said with truth, that the facts as hitherto discovered are not very easily reconcilable with any existing theory on the subject.

Proceeding now once more to another portion of the immense field comprised within the domain of geological inquiry, may it not be said with truth that recent discoveries on the fauna of the sea raise more questions and suggest more doubts than as yet, at least, they have furnished materials to solve? The close proximity of currents at different temperatures, each characterized by its special forms of life, and some of these so widely different as to amount to a wholly separate "facies," does not this involve in much obscurity the tests of contemporaneity in deposits which hitherto we have been accustomed to accept? Is it not evident that sediments exhibiting very great diversities in the forms of life may be not only strictly contemporaneous in time but also almost adjacent? And so, on the other hand, does it not equally appear that identical faunas may be very widely separate in time? And then does not the law of distribution of species present itself in a peculiarly unstable aspect when it is seen to depend on the comparatively slight physical

changes which must be often quite great enough to deflect a current? And, further, does not this complicate immensely the problems connected with the origin of new forms, inasmuch as concerning any given area it brings in the agency of constant immigration from without in the place of the agency of organic variation from within? I know that this immensely helps the imagination; but it is a help only because it affords a means of evading and escaping from the real difficulty involved. It is always very easy to suppose the introduction of modified forms into any area by immigration from somewhere else; but it is no satisfaction to those who seek to establish the law of specific variation to be compelled always to imagine the new varieties as having arisen somewhere out of sight, never in the field which is actually under view. And yet if we are ever to find anywhere a complete and continuous record of the history of life upon any given area of the globe, it must be some marine area, some portion of the surface which for a long period of time has been uninterruptedly occupied by the ocean. It is at least possible, and it has been surmised on high authority as even probable, that such areas are actually to be found in the abyssal depths of the existing seas—spaces which have been occupied by like depths of water ever since the period of the Chalk; and if this be so, then all the conditions for the perfect continuity of life would be present on that area, because, although there is some difference of opinion as to the physical cause to which the effect is due, the fact seems now to be established that a low uniform, or approximately uniform, temperature prevails at the greatest depths in every latitude wherever such depths exist. The particular theory accounting for that low temperature as a part and consequence of a general system of oceanic circulation which has been so ably expounded of late by Dr. Carpenter, is a theory which belongs to physics and not properly to geology. If I may venture to express any opinion on a matter which is involved in some difficulty and doubt, I should say that this theory is at least more satisfactory than any other which has hitherto been suggested. But whether that theory does or does not adequately explain the causes which lead to the permanent coldness of the bottom water at great depths, it is certain that such tracts must impose limits on the migration of species at least as effective as the highest chains of mountains, upon the land, and that they afford areas on which there is every reason to suppose that we shall find an uninterrupted series of the forms of life for a very long period of geological time. So far as the investigation of it has yet gone it does appear to carry us

back to some, perhaps to many, types which it was thought had disappeared. But on the other hand I believe there is as yet no evidence of specific continuity, but, on the contrary, indications of the same number of unbridged spaces as before. The voyage of the 'Challenger' may be expected to throw the most important light on these and many other points of deep interest to our science.

Lastly, among the questions on which geology has not yet succeeded in throwing any clear light is that question most nearly interesting to ourselves. For as geology, in the beginning of its research, passes into cosmogony, so, as regards the endings of its research, it passes into archaeology and enters even upon historic times. What light has it yet thrown upon the origin of man—the highest in that great class of mammalia to which physiologically he belongs? As regards other members of that class, although the spaces remaining unbridged are wide and large—so large as to leave unexplained the birth of specific distinctions—yet they are spaces which can be bridged, at least in imagination. The affinity between the Horse and Hipparion, between existing Ruminants and extinct forms which herald their approach, is at least an affinity which suggests a genetic tie; but no similar approach has yet been discovered to the master form of life. It belongs to geology to discover this. There is no other plea for failure except the "imperfection of the record;" nor can this plea be pushed too far, when we know that the record, however broken and fragmentary, is at least sufficiently complete to furnish, as regards other existing forms, that which it has not supplied with reference to man.

I am very glad, Gentleman, that I resign this Chair into the possession of a Member of our Society who has devoted special attention to this subject, and has put together all the evidence that our science yet affords on the early condition of mankind with an ability and completeness which leaves nothing to be desired. Some of the most eminent fathers of geological science in this country have, alas! now disappeared; and those who remain must desire and are entitled to well-earned repose. Among the younger men who are following in their steps, and who are illustrating geology by the application of much literary ability, and by the stores of a cultivated mind, there is no one more distinguished than our excellent Secretary Mr. Evans. He will be able, as I have not been, to follow and to reproduce the progress of discovery and discussion during the tenure of office which will now commence.

I hope you will excuse me, Gentlemen, for having done nothing

but indicate shortcomings and record doubts. The suggestion of doubts is not altogether an unimportant duty; and the definition of questions on which we are ignorant is a first condition of success in dealing with them. In scientific inquiry it is a wise thing, and a useful thing, and by no means a purely discouraging thing, to pause sometimes and consider with Mrs. Somerville "How little it is that we know."

